SKY FOR SOC REL, First semester '61-2

esp. for Stoner. Scored by Elizabeth P.

HASTY RESEARCH MEMORANDUM

THN
25-6 OCT 61

interviews

1) On reading all these ~~difficulties~~ I have the greatest difficulty seeing that Research Science will necessarily fix itself up in defense against all these strains. The phrase that recurs in the different Proposals for Analysis is that Science must find a way to 'protect its integrity'-- but we all know that the need itself doesn't produce the reform. (The experiences of Biology and for one humorous period, Linguistics in the Soviet Union bear this out. Why be optimistic?)

Naturally, we want to look for mechanisms that might effect changes in desirable directions. Then we have a problem partly because our data is synchronic and may mask changes.

2) The social system I notice maintaining its integrity is the Administrators. I didn't count anything but it seemed like Scientists thought administration was bad, administrators thought it was good. When does this socialization take place? Interesting trends may be operating here to keep the boundary distinct-- keep the young fellow out of administration by glares and remarks about losing touch with bench and blackboard, keep the other guys in admin. by letting them get out of touch.

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26 Oct 61

REPLY TO "Hasty Research Memorandum"

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The interesting problem, though, is why. Certainly some sorts of social systems [and, as you suggest, administration may be one] do act to protect their integrity; what is Science's achille's-heel?

I wonder if the SSofS isn't really a kind of social residual-category?--it flourishes only in the condition that we have known it, when it is otherwise ignored by society, and as soon as society begins to pay attention to it, the charm is gone and the SSofS disintegrates into an engineering-like profession.

If this seems true, we might propose that there is indeed a hierarchy of preferred "rewards" within society, and that professional recognition ranks mighty low on this list. XXXXXXXXX It isn't exactly Gresham's Law, but in this case as soon as some juicy rewards become available, everyone loses sight of the older ones.

Science may in fact be the social equivalent of an entropic system--it exists as a system only because its boundaries are maintained by agencies in the environment (just as a chemical reaction can exist in a beaker only because the beaker holds all the stuff together; break the beaker and the reaction ceases). In this sense, too, science may be equivalent to the juvenile gang--whose organization is possible only when there is mutual repulsion between its members and the rest of society.

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Ted Nelson
Soc Rel 301
24 Oct 61

Outline Fragment:
DESCRIPTION SPACES and their relation to phenomenology, cognition, induction and coding-languages.

I. Two notions of descriptive, representative and conceptual space.

It is assumed by most social scientists, following a generally positivist train of thought, that descriptions and models may regularly be represented in a "space" which is both stable and defined. Hence a very great effort is made to set out precisely the frame of reference, define terms with exceeding caution, etc. They assume what we may call "stable" or "crystallized" space, where all the dimensions are known and defined in relation to one another. e.g., Cartesian space

Let us, however, imagine another kind of space, in which all relations that are described need not be precisely set forth, nor all ambiguities clarified. Let us call this "semi-connected space." Let us assume in addition that all statements of relation or existence in semi-connected space are set forth in symbols which do not have any ordinary meaning, so that they are not tied down as to their future use.

II. Some advantages of semi-connected space.

1. Semi-connected space has considerable advantages for models that are changing. It may be that a model-builder will want to change parts of his model-- for instance, by distinguishing two things that he had previously thought were the same, or by finding a similarity between things whose similarity he had previously pointed out (subsumption.) He may wish to change the nature of certain dimensions-- e.g. deciding a continuum is really a dichotomy, a Guttman scale, etc.-- or manipulate dimensions, as by rotation. Many of these operations involve the creation of new dimensions (and finding cutting-points for them,) or the destruction of dimensions. The important point is that the model-builder can maintain configurations in his model while he modifies it.

2. The model-builder can also ignore problems of completeness and economy of expression. He need not say whether two dimensions are the same or different, whether a given

dimension is really "unitary" or a package of others; whether it is redundant or leaves out too much. No items need to be assimilated into the whole or related to one another unless the model-builder is ready to do so. Rotation, compression and collapse of dimensions is not necessary.

3. The model in semi-connected space may have incompleteness, ambiguity and omissions. How it applies to possible cases, how it relates to other models, and what relations pertain in various parts of it, need not necessarily be stated either.

4. "Background" and "foreground" need not be separately distinguished; background itself may be omitted. Some models, of course, are entirely clear about the universe to which they apply. In that case we may take the background, or "frame of reference," as actually part of the model; it is further specification of the universe, on further dimensions.

III. Some examples of such systems, which can only be be properly represented in a semi-connected space.

1. The Phenomenal system. Conceptual mental experience, whether in words or in senses or feelings, could only be properly represented in a semi-connected space. In other words, an organism feeling (or 'sensing') new contrasts and comparisons between things, new categories and continua (where before there were just individual experiences and things,) is operating in such a space. Apparent connections fold and unfold the entire space in important ways. New distinctions come about, etc.

Perhaps an example would help here. Imagine a game played between a chef and someone who knows nothing about cooking. The chef's object is to mislead the subject as much as possible, while the subject's object is to learn as much as he can about what spices there are, how many, what are the differences between them, etc.

By and large the subject will discern a number of distinctions, possible axes of comparison, etc., which will change piecemeal -- but in such ways as to change and reorganize his whole notion of the universe of spices.

This is the realm of "conceptual structure" = which comes before the more formal setting up of a model in which it can be substituted for the model itself!

2. The cognitive system. Here the elements and connections are much more precise, statable in propositional form, etc.; but it must be emphasized that the difference is one of degree. The phenomenal system is less precise and more prone to more simple operations, but they are of the same general type.

3. Some elaborations of these types.
   a. The phenomenal system seems to have processes like the following: drift, where connections, contrasts, etc., slowly move along some dimension; decay, attention and pivoting, where certain of these elements are 'fixed', or 'weighted,' in such a way that the rest of the entire system may change around them, or decay. When large-scale shifts take place around certain of the elements, it may best be called "pivoting."
   b. The cognitive system is more prone to crystallization, whereby specific relations are fixed in propositional form. The processes of drift, etc., are somewhat modified by this.
   Similarly, evolved cognitive systems come to allow only certain operations to take place in their semi-connected space. These operations are similar to, and may be the same as, what is usually called formal "logic."*

You are here constructing a model - in static space - of thinking! A. Rapoport

IV. SCIENCE AND PROGRESSIVE COGNITIVE ENDEAVOR AS SUCH A SYSTEM.

There is considerable controversy about just what constitutes "science," and as to the precision with which its elements must ultimately be crystallized, and the form they should take. We have distinguished two major expressions of this view:

1. The metaphysical thesis, as expounded by such positivists as Karl Hempel, is that in some ultimate sense science must finally become a completely crystallized cognitive system, expressible in some condensed and axiomatic form.

What we consider to be a full refutation of this thesis may be found in the article by Scriven, "Definitions, Explanations and Theories," in Feigl, Scriven and Maxwell, Minnesota Studies in the Philosophy of Science, v. II. Scriven points out that in the best-developed and most predictive sciences there are still indeterminacies and imperfections, and if the "best" sciences cannot reach this goal, there is no point in holding it out to the social sciences.

2. The programmatic thesis, advanced by Zetterberg in On Theory and Method and Sociology. The theory here is that in all stages of science an attempt should be made to proceed with fully explicated definitions, theories and axioms-- in the most compressed form possible.

The best answer I can see to this view is that theories of this type are not "debuggable" (an expression in computer terminology.) When these theories are found faulty, they are almost completely insusceptible to correction. Not only the proportions of things in different categories-- Mr. Zetterberg xp uses this example, being versed in survey data-- but the theoretical categories, the indices, and every possible link in the theorixxxmay bex faulty-- or xxxxxx more exasperatingly, the theory may be substantially true but because of some one or two faulty elements, be "disconfirmed" in an empirical study. Zetterberg does not allow for these difficulties.

VI. INDUCTION and semi-connected space.

The intent of science is to find out what things remain constant while otherx things change. They may be physical objects, relations, etc. Unfortunately, discovering the entities of concern, thex invariants, is often as much a problem as discovering the changes that take place.

Example: in the Indo-European languages it was found that certain things remained constant from language to language. Ixxxxx Not the parts of speech or the meanings or the sounds; but rather clusters of sound-- sounds recognizably distinct from other sounds, whose distinction was preserved; and families of meanings associated with chains of sound.

Example: in the study of attitudes there is still no great conceptual clarity in the meaning or validity of different scales. What scales of valuation ultimately distinguish societies, or whether any do; what universals connect the conceptual worlds of what groups of men, if any do, are not certainly known. Most social p scientists are predisposed to look for some canonical set of scales which will do all the important descriptive work. We doubt, however, that this is as likely as they hope. For it may well be that resemblances of conceptual schemes between any two sets of people could be used to "create" a dimension of inductive resemblance and disresemblance-- not an ultimate, but a distinguishing one.

VII. CODING-LANGUAGES, induction, semi-connected space.

It would follow from the above that one important concern of the social scientist is what things he should be observing, that is, "coding"-- what things he should be looking for, and what differences he should be noting.

This proposition is fundamental: without enough categories, it may well be impossible to discover what changes are taking place. If the object is to find out"across all dimensions" what

things
~~changes~~ are stable and what changes are taking place, it is no help if ~~only~~ only enough categories are being used to record the similarities-- for instance, if the brothers Grimm had used the same alphabet for all their investigations, ~~t~~ there would appear to be very little difference among the Indo-European languages. (Of course, this is largely a problem of the ~~m~~ turn of a particular subject-matter, ~~and~~ the availability of evidence, and the costs of making particularly fine distinctions. There may be so many dimensions of data available as to make the whole study very unwieldy.)

An amusing sub-problem of coding is the effect of phenomenal schematics on the individual coder. ~~D~~rift, ~~fixation~~ etc., may have the effect of causing certain categories to evolve over time, unless precautions are taken to keep all relations in the system constant.

Any "object" — including an interview typescript — can be defined in an infinite number of ways. In other words, To say that we should code things in as many dimensions as possible means an impossible task. What criteria shall we use to restrict the # of categorizations used? I think the main ones are: 1) a sense that there will be sufficient cases in any one to be useful for analysis, and 2) the intuitive sense that the category will be useful in explaining whatever dependent variable you have in mind. In other words, no one can ever "milk his data dry"!

26 Oct 61

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28 Oct 81

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Ted Nelson
Soc Rel 301
2 November, 1961

STIPULATIVE SCHEMATIC CORRESPONDENCE.

Schematics are terms or sets of relations which can be set in correspondence with something else, or "mapped" onto a "domain." The domain can be material or ideal, specific or general, tangible or abstract. The schematic is an idea or set of ideas. We will here occasionally use the terms "model," "presentation," or "descriptive presentation" to mean the same thing.

To simplify this discussion, we will treat schematization as a sort of problem-solving game between two people: the "theorist," who is a little too attached to his schematic model of something, and the "objector," who tends to doubt that it is a good model. The theorist thinks that his schematic fits well when applied to a given domain, and the objector thinks that it does not.

In these writings we are fundamentally concerned with this conflict. We may call the attitude of the objector "schematic uneasiness," and that of the theorist "schematic righteousness." The first is the attitude that a description is in some way inappropriate, and that its terms, distinctions, dimensions, currencies, etc., seem to be unjustified. The second is the attitude that it is not necessary to examine the grounds on which the schematic is based, and that its use is justified for a-priori or prima-facie reasons.

It should be noted that these are not arguments related to logic, but rather to theory. Neither person in such a conflict needs to employ deduction, and neither participant is thinking "fallaciously."

It is basic to these writings that schematics can be unsatisfactory in many ways. Sometimes they will be unsatisfactory in a way that their proponents will acknowledge, if it is pointed out to them; sometimes they will be unsatisfactory in certain abstruse senses that the theorist will not consider relevant. Generally this unsatisfactoriness will come about from the failure of background conditions to remain as the theorist has supposed. Various problems of description and conditions for noticing, however, may cause this failure of background conditions to be ignored, so that the theorist may continue to believe in the unaltered applicability of the schematic.

To make this perhaps a little clearer, let us discuss what we regard as some bad examples of schematic application. Let us call this "stipulative" application, where a schematic is inappropriately applied. In the examples it will be noted that we do not distinguish between cognitive and normative, that is, between schematics of recognition and schematics of disposal or decision. We believe that the same approach applies to both kinds. In either case the schematic is used as a "cookie-cutter" where it should not be.

1) The naming of branches of understanding and subject-matter.

In the Middle Ages it was believed that there were only seven subjects of knowledge. A group of four in particular were called the "quadrivium," and the three others were called the "trivium." It was believed for a long time that any knowledge would automatically fit into these categories-- and, in fact, it did, with the distortions being hidden. For example, the natural sciences were called "philosophy" long after their relation to traditional philosophy had become attenuated. /?/

Similarly, the designers of library cataloguing systems, such as the Dewey Decimal and Library of Congress system, seem to have felt that they were dividing knowledge along some eternal lines that would remain appropriate indefinitely.

The categories in Roget's Thesaurus are similar. To the contemporary reader, these categories seem to embody a view of the universe that is out of vogue.

Academic departments even today reflect slightly old-fashioned views of the division of rational inquiry (although it may be predicted that there will always be some lag.) However, no matter what the names of the departments are, a "place" within the existing organizational structure can always be found in the existing setup, e.g., for a new seminar. New discoveries and theories are quickly parcelled out to specific traditional divisions of understanding, and difficulties of fit-- as, for instance, with Lotka's theory of population and ecology-- may not even be noticed for some time.

2) The use of terms related to some dynamic theory, where it is not clearly appropriate.

Herbert Spencer's notion of "evolution," for example, was presented by him with many specific examples, each of which illustrated in detail some aspect of his intricate model of evolution. The fact that these particular illustrative examples tend to have only the attributes he is describing at a particular moment, and do not correspond well to the rest of his model, is not immediately noticed.

Freudian theorists notoriously tend to apply their terminology-- if not in specific assertions-- to every conceivable field. At least one example is admitted by everyone to be absurd: a satirical article, published by a reputable psychological journal, which described chemical compounds as "masculine" and "feminine" depending on their molecular shape. This was related to the differential effects of two different classes of tranquilizers. /?/

3) Administrative divisions tend to be applied as much out of historical inertia as for their functional reasonableness. For instance, the organization of a navy research unit or maintenance station will be based on the conventional divisions of rank and function in the navy, while the identical research or maintenance task will be differently divided and supervised if under the control of the army, the Department of the Interior, or a corporation subcontracting government work.

(It should be noted that the conventional criteria of 'rationality' and'efficiency' are not directly applicable. The criteria for the appropriacy of these schematics to problems of administrative division are varied and diffuse.)

Indefinitely many examples may be found to show that schematic application can be curious or inappropriate. It remains to point out the extent to which schematic stipulation is possible. At this time we wish only to make a strong assertion, and hope it whil be taken on faith: that stipulative application of schematics is possible to an enormous degree. Frequently this is done by the theorist in the guise of analytical necessity: it is presumed by the theorist that there is some logical need for the schematic to be applied in the way he proposes.

There are two clear cases in which this assertion is most reasonable. These are cases in which the theorist proposes a schematic which can be clearly transformed into other terms, and do not mislead or necessarily lose information by being cast in this schematic. These are partitioning, in which the domain is broken up into subsets, and it is clear that other subsets are possible -- for example, "men under thirty and men over thirty;" and dimensionalizing, which posits possible directions of variance in the domain-- for example, age and sex. (The first dimension here is continuous, the latter divided into discrete categories.)

A third case, the complex term, is less clear-cut in yielding analytically necessary and justifiable schematics. Some examples are the distinctions between heredity and environment, actor and situation, ego and id, line and staff, procedure and policy, morality and discretion, God and Mammon (metaphorical.) What might be left out of these schematizations cannot be ascertained from the terms themselves; both their application and their translability are uncertain from inspection.

As a rule, however, schematics are not explicitly introduced with their basis stated for examination. A theorist may take it for granted that a schematic is applicable. Having identified certain parts of a domain with parts of the schematic, they will go ahead and look for another explicitly. For instance, if Parsons identifies a thing as a social system, he is automatically directed to look for the A-G-L-I parts, expecting that each part will appear, and be something familiar. Looking at this one way, it is a "logical" requirement of his system; however, this supposes premises and

deductions which do not interest us at the present. Looking at it another way, we may call it a "schematic requirement."

Another disguised way that schematics may be introduced is in the form of "terms," whose schematic implications appear only gradually. In this case the theorist introduces the terms in such a way as to give the impression that they do not carry any theory, but they act effectively like the Trojan horse, and permit assumptions to be introduced without discussion. For example, the theorist may introduce the term "ultimate value," and thereby suggest that there is necessarily such a thing-- without actually examining the question of whether there are "ultimate" values, things which most basically characterize an individual's structure of beliefs or respects, or distinguish them from another individual's, or lie along a hierarchy shared conceptually with other members of his culture. The term "ultimate value" suggests any or all of these without examination. The theorist here suggests that his terms are not theory-bound, and allows their schematic implications to enter the discussion unacknowledged, perhaps under the assumption that they are analytically necessary simpliciter.

There is an assumption which underlies any schematic assertion. Simply stated, it is this that the structure of the domain remains constant regardless of certain changes in various possible directions. It may be assumed further that various measuring tools, and the things these tools are based on, remain constant too.

Often the application of a schematic in a limited number of cases will be extremely clear-cut, and it will be extrapolated to new kinds of cases; it may be an analytic model, such as the ones mentioned above, or it may be some measuring tool, reliable in a number of previous cases, which is applied elsewhere under the assumption that it is still measuring the same thing. While it may be reliable in the usual technical sense of test reliability, it may not be appropriate in the larger sense. If, for example, it had been reliably found that the question "do you feel that the government is against you?" correlated well with certain characteristics among high-school students, we might have reason to expect differently if we ask the same question of inmates of Alcatraz.

In these few pages I have only been able to give some examples of the problem of schematic application. Later I hope to relate this to the general problem of induction, and the repair of an inductively derived model; to problems of the observation and representation of likenesses and differences; and the representation of this inductive process in semi-connected space.

TWO APPROACHES FOR THE STUDY OF LOGICAL SYSTEMS;

With particular reference

to scientific models and systems for doing.

Theodor Holm Nelson
Social Relations 301
January, 1962

This paper has a curious history. The topics mentioned here have concerned me for a great length of time, and I have had an inordinate amount of difficulty attending to them within the legitimate academic framework. The present paper presents the gist of some fifteen or twenty papers that I have written since 1956, many of them only peripheral to the courses in which they were offered. Whatever the shortcomings of this paper, I believe it more clearly presents the broad outlines of what I have been trying to say than anything previous.

I hope that certain imprecisions, and the lack of detailed citations, can be overlooked. My intention is to show a certain approach for the study of logical models, apply it in some detail to the problems of models and theories in the social sciences, and suggest other applications in sketch. Further developments of this idea should of course take full account of the relevant literature in these possible areas of application, but in the present treatment I have as yet been unable to do so.

This will be a discussion of the structure of logical models. I will take the "literal-and-minimal" approach to "scientific theory" as the paradigm, and try to show it to be an inadequate account. I will then briefly outline some developments of these ideas, both in the social sciences and

in "systems analysis," summative, interdisciplinary branch of engineering called "systems analysis."

Thus I hope, first, to indicate some ways in which the development of minimal axiomatic formalizations, stemming from modern mathematical treatments of logic, has provoked some mistaken views of the nature of scientific knowledge, and (more unfortunately) mistaken approaches to social-science research.

The alternative approach, which might be called the "network-configuration" approach, suggests a number of applications, both for procedure in social science and for certain areas of development in the study of methods for doing things.

This paper will in general try to present an approach to the study of logical systems which is somewhat different from the customary approach. By "logical system" here, I do not refer to particular rules of inference or operators in different formulations, for instance, of propositional calculi, but rather referring to sets of descriptive connections among elements of sets. While "descriptive system" might be in some contexts here a more helpful term, in others it would not make sense, so it will in general be avoided in favor of "logical system."

The first approach to logic we will call the "literal-and-minimal" approach (L&M). This is characterized by scrupulous attention to the structure of deduction, the identification of logical primitives, and the precise explication of all assumptions.

We will call our approach the "network-configuration" (N-C) approach. Roughly speaking, this approach seeks to study different aspects of the structure of logical (or descriptive) systems as general configurations, without necessarily decomposing them into particular formulae or reducing them to minimal sets of axioms.

In following this approach, we may also be interested in the relation of such logical systems to similar possible logical systems, alternative arrangements of the same or similar elements and sets.

We consider the network-configuration approach to be in many ways an interesting and significant way to deal with certain problems which tend to be ignored by the usual approaches to logic.

In this paper there will be no quarrel with the impressive knowledge that has been gained by the use of the L&M approach in the formalization of logic, the reduction of mathematics, or the study of axiomatics. We will, however, assert that this approach has led to some unfruitful formulations of the nature of science and procedure. The other applications of our N-C approach will by and large have little to do with conventional formulations.

The structure of the paper will be the following: we will take an area in which the minimal-and-literal approach to logic has had considerable influence, that of the nature of theory and procedure in the social sciences. Specifically,

we will take the model of theory proposed by Cohen and Nagel, and contrast it with an alternative model suggested by the configurative-network approach. We will then treat the Cohen and Nagel model as a model of procedure--as I believe they intend it to be taken--and contrast it with a much looser network-configuration model which we derive from the model of theory. Mention will be made of actual work and strategies in the social sciences which illustrate these problems.

The remainder of the paper will be of a suggestive nature. We will try to indicate some of the applications a network-configuration approach might have for other problems in the social sciences, in logic, and in systems analysis.

A word on the terminology of outlooks, orientations and problems that will be used in this paper.

We will consider "theories" and "models" to be roughly the same thing.

We will mention two approaches to logic, which lead successively to two different accounts of scientific models, two different procedures in scientific research, and (most remotely and loosely) two different strategies of research.

Among the topics of interest under which these approaches may be considered are Extension; Correspondence and Mapping; and Network Properties. I have chosen these

because I expect they are of more general interest than merely for the model of social-science procedure. Further "considerations" in its favor will be advanced, however, which are specific to the problems of social science. In the further applications of the N-C model I believe the three perspectives will be of further interest, although there is no room in the present paper to discuss them in detail.

Several other terms will be used here in rather special senses. By "predicate," "predication" or "specification" we will mean, loosely, a logical connection between elements, or between elements and sets (such as categories,) or between categories, that in some way specifies a relation between them.

By "network" we shall mean a system of entities in which there is more than one predicate. For instance, "George is Bill's brother, Anna's son, and the father of Sam and Emily, but he is in no way related to Ellery" is a network.

By "definition" we shall mean some predicate connecting two or more entities which is directional, going from an entity which is logically prior to one which is contingent.

The following is an account of "scientific method" and "scientific procedure" which may be said to have derived from the literal-and-minimal approach to logic.*

* While I think the influence of this account in something like the present form can be attributed to Cohen and Nagel, An Introduction to Logic and Scientific Method, it is not to be found explicitly in any one place there, at least in the abridged edition. Hans Zetterberg (On Theory and Verification in Sociology) has stated it more directly as applied to sociology.

A hypothesis is not scientific unless it fulfills the following criteria:

1) It has been clarified and made explicit.

2) An attempt has been made to set it forth in a minimal axiomatic form. This form should begin with axioms, which are definitions or statements of the most elementary principles involved. From there it should proceed by deduction to theorems, statements of the consequences of these axioms.

It is further held in this view that

3) The theorems should now be tested;

4) The axiomatic reduction will now permit the researcher to find those axioms which are incorrect and change them to conform with the data.

As an alternative, I will undertake, perhaps rashly, to develop an account of correct theory. Then I hope to show why, although this L&M account might produce such a theory, I consider it overly restrictive. I will endeavor to show that while a correct theory may be cast in the L&M account, a partially correct theory perhaps cannot; and if the object is to expedite the development of correct theories, then the L&M suggestions for procedure might be a hindrance. There are alternate accounts of procedure which would be disallowed by the L&M model, but would be allowed by the N-C account, which in several respects might well be more practicable.

The first formulation of our N-C account of a true theory will be the following: A theory is a descriptive model of the stable patternings within a universe. By "descriptive," we do not mean that the theory is the pattern itself, but some formulation of predicational connections describing it. By "stable patterning" we mean that the theory will describe some features of that universe which persist or coincide with other features. By "within a universe" we mean to restrict the theory's scope to some extent.

Let us now make a second, more precise formulation. A theory is a network of predications, some of which have coincident or co-mobile correspondence to some extensional universe.

Perhaps the most unusual aspect of the account is the phrase "coincident and co-mobile correspondence to some extensional universe." Let us take this apart. By "extensional universe" we mean that the theory is <u>about</u> something else, not merely a set of ideas, e.g., whose logical features we wish to examine. By "extensional coincidence" we mean that some predicates will have the same extension as other predicates, as in "The Vice-President of the United States is also the President of the Senate."

"Extensional coincidence," in the sense above, is very like "correlation," but not the same thing. As used

here, "extensional coincidence" is an all-or-none property, while correlation is graded, taking place in various degrees.* Let us take two examples of this, the first a category, the second an entity. For the coincidence of two categories, "Those invited to President Pusey's dinner were members of the Graduate Student Council." The coincidence of remote predicates is of special importance. If the predicates are at some remove in the network, we call them remote; thus "The father of my mother is also the head of the company that examines claims for shipboard damage" might be considered such an extensional coincidence of remote predicates.

An interesting property we might mention here is that of boundary coextensiveness. For instance, the area sometimes called "Delmarva" is coextensive in boundary with the states of Delaware, Maryland, and Virginia. Another interesting property is what we might call "co-mobility," the property of moving together on some dimension or set of dimensions, like space and time. For instance, the suspension of water droplets in air is co-mobile with the reflective properties of a cloud. (An entity we wish to call "unitary"--say the "ego" in Freudian psychology--is generally expected to have some co-mobile properties other than our propensity to call it by a single name.)

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* While the above model can be adapted for imperfect correspondences, such as those made necessary by principles of indeterminacy, we wish to associate the term "correlation" with a certain specific style of procedure, below, and so use the other term here.

Another property of the account deserving mention is the property of correspondence. A given network of predications could be applied to the same universe in different ways unless there were some set of criteria or "bridge predications" which relate the model to the universe of discourse.

Let us now discuss the reasons for considering the network of predications within the account; first, reasons for considering the network at all, and then reasons for discussingtthe network as a whole.

Any model is inherently vague if it merely assigns predicates to elements without explaining those predicates. Thus, "cows are ungulates" tells us nothing, even if we have bridge-predications that enable us to identify "cows," unless there is some way that we may turn to find out what "ungulates" are. For this reason we require that there be some wider predicate network that will further specify the specifiers. This is <u>like</u> saying, "only if there is a definition (or operational definition) is a term meaningful." However, this is not the current assertion. We are trying to find here some notion of "significance" which will be based on the extent and precision of interrelationships, rather than merely having a recursive definition available for every term, centrifugally on and on. For this might end in "undefinables"--things for which no precise explication or criteria can be given--or in circularity.

In a "network," as the concept is being developed here, the notion of "definition" may be systematically unclear. We said earlier that a "definition" requires an order of logical priority between the thing defined and the definition. However, considering a given nexus in a network, of the form "A is B," there may be no way evident within the system to assign this logical priority. Which of the two elements A or B is prior, and which is contingent, may be purely arbitrary.

This is not to say that it is necessarily arbitrary or useless to assign logical priority. One of the most important uses of the definition is in expository order, going from terms which have already been introduced to those which have not. A kind of logical priority also describes the anchorage of a new term in terms of an old one, or in the context of current knowledge. But the attempt to anchor one term to another may be unreliable, as we shall see.

Another point must be made in regard to the extent of recursive definition available. This is the point that mutual definition is some specification. In circular definitions, the network of connections does in fact tie all terms down to some extent; the effect is reciprocal among all the members, even if there are no "bedrock" primitives at which the relationships end. In this sense, we would assert that there are not necessarily undefinables within a logical

system. Logical "primitives" of this kind can only be distinguished if there is also some basis for distinguishing between defining and contingent terms.

The least important thing about a network, on this account, is the question of which predications are defining. In any case the hypotheses of extensional coincidence will remain the same.

It should be evident that there are probably many ways of expressing such a network of predicate connections. While the L&M account advocated the search for a minimal set of axioms from the number of possible alternative lists of propositions which could set forth the model adequately, this one does not. Instead, this account would advocate some sort of larger listing, making no attempt to avoid redundancy and may list several propositions which could be deduced from others in their own right.

However, thus far the difference between the two accounts does not seem very large. One emphasizes the minimal expression of the model, the other emphasizes the overall characteristics and patterning. However, strong disagreement appears when we turn to procedures for verification.

However, it should now be pointed out that any model is committed to correctability. Therefore the procedure must be designed to undergo in some way what we may call the inductive cycle, the sequence of supposition, test and correction by further supposition. The question is then

what procedure will be best suited to this in its various stages, or if different procedures have different advantages, what these are.

It will be recalled that the L&M account advocated the isolation of minimal axioms sufficient to derive the entire model. It has been mentioned, however, that a variety of such minimal sets may be available (from which, on the L&M account, we should select on the basis of such features as conciseness and deductive elegance.)

The procedural difference centers around the problems of disconfirmation and retrenchment. The L&M procedure can be criticized roughly on these grounds: it provides little help in reformulating the model to deal with disconfirmation, especially in those cases where a model something like the original is more appropriate.

The L&M procedure assumes that the model's failure will be localized in one or a few axioms, but there is no reason to believe that this is so. Consider the following fragment of a model:

1. English muffins are a nourishing food.

2. English muffins are served split in two, with hot melted butter.

3. English muffins may be purchased at a baked-goods counter.

4. English muffins are caught off the coast of Maine.

5. English muffins are green until they are boiled, when they become red.

Suppose we had deduced the following theorem from this model: "English muffins are caught off the coast of Maine, and packaged in cellophane." We find this, on empirical investigation, to be false. Which axioms are false? Parts of several are false. How may the model be repaired? We could either substitute "lobster" for "English muffin," in which case (2) would be false, or look further and discover that (1) and (2) were true for English muffins, (1), (2), and (3) were true for lobsters.

By this example we intend to show the difficulty of localizing mistakes in a set of axioms. Two disadvantages of this example for the present exposition are that the propositions are disjunctive, it can be repaired with relative ease. However, our assertions should seem reasonable: that the ability to isolate and repair incorrect parts of a deductive model without largely overhauling it--with whole new sets of axioms--will be largely fortuitous.

We will now look at two "procedures" suggested by the N-C approach. The first "procedure" is fictitious, since it provides no keys to actual research behavior, but it will serve as an introduction to the second procedure, which does.

We will call this the factorial procedure.

Any number of past facts and relations may be found among concrete objects. Thus a certain cat may be male,

white, blue-eyed, live in Cambridge, eat Puss-in-Boots cat food, and be totally deaf. Suppose we want to account for his deafness. The question will then be, which facts about this cat (among the myriad we could ascertain) will account for his deafness? Can we isolate predicates, near or remote, that will extensionally characterize this condition? The factorial view would suggest that we take all the deaf cats in the world, and "factor" their characteristics until we find those particular ones which are determining. Thus we would ascertain (owing to the choice of example) that all male white blue-eyed cats are totally deaf.

This factorial model could likewise be extended to relations as well. Thus we might learn alphabetical order by "factoring" the names of the authors of the books on General Reserve, or we might ascertain Newton's Laws by "factoring" (to a certain degree of fineness) the movements of the planets, the comets, and all falling bodies.

This is not metaphysical, but truistic, and of course gives us no concrete method for actual research.

However, now let us suggest an abbreviated factorial procedure, which takes into account the economics--that is, the limited resources--of data-gathering capacity, experimental capability, attention and processing ability.

This procedure would advocate, not investigating and factoring everything at once, but attempting to preserve the pattern of the theoretical network at hand.

Suppose the model has received disconfirmation. How now may we improve its correspondence to the universe at hand? By a series of quite varied discrete moves, changing any part of it that seems reasonable: the denotative or bridge connections, the connections within the network; by adding or changing constructs and entities, extrapolating new dimensions, intercalating new postulates. These may be called "logical" moves (to be kept distinct from logical operations,) since they are performed on logical systems. In the situation described above their function may be to preserve configurations, configurations of insight and supposition.

If the model is set forth in the extended form mentioned earlier, which ignored minimality in order to set forth extra propositions in a redundant fashion, ways may be apparent to improve the model as a modification of the original pattern. This, it will be recalled, is what the L&M procedure had considerable difficulty doing without retrenchment. However, by explicitly allowing in advance that such actions may be necessary, we have perhaps made the problem simpler. The extended form of a model should preserve more of the original ideas for subsequent modification, since its improvement does not hinge on the strict maintainence of deductive continuity from proposition to proposition.

Earlier we mentioned "the inductive cycle," the sequence of supposition, test and correction by further supposition. We have indicated that the "permissive" model is more amenable to correction. Let us examine briefly another aspect, the problem of coding and processing.

The question of what forms of test a model is amenable to will be entirely contingent on the nature of the subject-matter. So, too, will the quantity and kind of extra information that may be obtained and held to facilitate the improvement of the model. In part this is a function of the economics of research--whether a little more information costs a great deal, or vice versa. In part it is a function of the directions of the conviction and unease the researcher has about the model, and the directions in which he will plan on, or allow, the information to be gathered. In part it is a function of the development of ways of re-processing the data, as in Lazarsfeld's techniques of "elaboration" of survey materials. Thus we may consider this problem to be formal, inspirational or both, depending on the area of research.

It is in order to mention at this point two strategies of research which bear a loose relation to the approaches, accounts and procedures which have been discussed above. Both of these strategies may employ either the permissive or axiomatic procedure. However, they bear other relations to the models described above.

The categorical-correlation style of research is roughly as follows: certain categories (for instance, types of individuals in a survey sample) are distinguished, and other categories (for instance, responses) are matched against them in search of correlation. This method has often been spoken of as "scientific" because the initial independence of the terms resembles the "primitive elements" of axiomitization, and the theoretical connections sought between them are easily phrased as postulates. To take only one example, Festinger follows this model in The Theory of Cognitive Dissonance.

The other method is that of configurative similarity, in which some general resemblance among situations or entities is observed, and general dynamic or systematic processes apparently common to all is investigated. Two outstanding examples of this procedure may be seen in the work of Freud and Parsons.

It should be clear that these procedures may easily lead to different emphases, especially in the research plan of the originator. The categorical-correlation method will emphasize confirmation and disconfirmation, sometimes to the extent that it may seem hurried or remote from the original ideas, far-fetched and too susceptible (in the disparate tests employed) to alternative interpretation. The method of configurative similarity apparently encourages its ad-

herents to a lengthy prolongation of theoretical analysis, delineating the entities and describing the relations among them. Characteristic criticisms of this strategy deal with the problems of distinguishing logically central aspects of the model (on which there is likely to be disagreement) and criticisms of the descriptive apparatus and methodology.

However, it seems to me that the commitments to confirmation felt by these men or their adherents may be taken as equivalent. There is no doubt that operational testing of dynamic theories is possible; nor can there be doubt that judgments of configurative similarity must figure in correlational studies from the very beginning (or else no two things could be put in the same category; and the configurative theorist may envy the correlationist the easy criteria by which his material lends itself to this.)

The classic example of research in the social sciences which successfully employed the permissive procedure is the research reported in Roethlisberger and Dickson, Management and the Worker. The original hypotheses dealt with the effects of different lighting conditions on the output of workers. Because output rose under all lighting conditions, the study was expanded into a study of the effects of primary-group relations in the factory setting. There could have been no hope for repair of the original hypotheses within the context of the study.

So far in this paper we have discussed the applications to the social sciences of the procedural model which stems from the network-configuration view of logic. Before broadening to other uses of the network-configuration view, we will summarize and broaden some of our assertions for procedure.

First, we wish to mollify "operationalism" as an ideology for research. If we identify it with the L&M procedure described here, we find that it is overly restrictive as an account of models which may be successfully modified in the light of disconfirming evidence.

Second, we would like to argue briefly for the anchoring of natural-language terms. There is no reason, as Pareto said, that a model cannot be formulated (in either procedure) with letters and subscripts; but there is no reason to expect, or ask, social scientists to do so. Such usage is clumsy, and there is no forgetting that we are dealing with the human world.

On the other hand, it should be remembered that a large number of terms in the social sciences have meanings which are quite clear in natural usage, and because understanding of human situations did not begin with Comte, there is reason to expect such usage--though not necessarily precise, entirely consistent or susceptible to definition--to be reasonable and correct for describing many phenomena of life. These are cluster-terms, for which criteria and many non-defining

predicate connections exist. "Love" is one such term. There may be disagreements about "true love," "puppy love," "falling love with love," and the problem of finding out whether one is in love; but to try to use it for something else than the general range things we call love would be inadmissible. It is an interesting question as to whether theories should or can be constructed which use such concepts in their natural-language senses; it may be, after all, that the coinage of terms that sound like jargon to the layman--like "cognitive dissonance"--is the only alternative. On the other hand, the general acceptance among sociologists of the term "sentiment" for such things as love indicates that the other might be possible to some extent.

Third, we would like to make certain suggestions about coding. Particularly with modern data handling methods, it should be possible in the near future to take in and handle a good deal more information--especially in the processing of interviews and similar materials, since questionnaires already tend toward the limit of subjects' patience--than has formerly been possible. Special attention should be paid, then, to the use of more categories for coding, and the evolution of categories as various proportions of the material are processed, followed by the re-processing of earlier material. The development of computer programs for this work should make it simpler than it sounds.

Let us now open the discussion to some broader suggestions that the network-configuration approach might have for the social sciences.

One example is the notion of the logical "move," described above as a way of improving a theoretical model. The logical move resembles some of the actual processes of thought: the improvement of a mis-matched cognition. Similarly, the "analytic mode" of attempting to establish a correspondence (as employed, above, by configurative-similarity strategists) is similar to the everyday psychological phenomenon of "insight." Either of these processes requires the thinker to hold aside possible contradictions, or avoid processing them until the insight has been clarified.

These might be thought of as network-configuration ways of looking at processes of thought. They do not directly resemble other work in the logic of cognition, (such as Piaget's work on the development of logical operations, Bruner's work on the development of concepts, Osgood's work on conceptual correlation, Festinger's work on cognitive dissonance, or Cassirer's writing on progressive conceptual integration. However, I believe it may be thought of as a complementary mode of analysis. If any of the network-configurative ideas can be formalized (see below,) it may supply a sort of descriptive model, a logical summary of cognition capable of handling integrations (as when sets of relationships are increasingly seen as a whole,) and latent contradictions.

I think that something like this view might also present an interesting account of certain features of measure theory, particularly some of the remarkable properties of measurement in the social sciences. It is rather amazing, at least to some people, how extremely crude measurements of vague phenomena can be developed into convincing results (for instance, Inkeles' recent work on the happiness of workingmen around the world) while other, well-understood things may resist measurement; for example, calculations about what other people are thinking. (However, this latter is a challenge, and it is presumptuous to predict what things will be measurable in the social sciences a few years from now.)

This particular problem of measurements reminds us of the topic discussed earlier, the similarities of configurative networks in which correspondent parts cannot be easily distinguished, perhaps not even found by known formal methods. One such similarity is the similarity of output, as defined by remote predicates which have not yet been isolated. Perhaps we may think of this as isomorphic to the problem of awkward or equivalent measurements, as formulated above.

Let us now discuss a few interesting leads which the N-C approach might give us in the philosophy of description.

For instance, the foregoing exposition gives us a sketch for a treatment of the induction of resemblances in

general. In this case, it might be formulated roughly as the process of configurative similarity plus the isolation of remote predicates--and "similarity" would then be a property of a network which falls some degrees short of being a scientific model, because the remote predicates themselves isolated lack significant extensional coincidence.

The remarkable persistence of relation features--for instance, in the history of ideas--is also of interest in this context. Systems of ideas can be tremendously alike and yet tremendously different, and the impact of ideas upon one another is of interest both to sociologists and to historical philosophers. This "sameness" of configuration may also prove itself amenable to some kind of N-C analysis, partly as resemblances in general, and as dynamic systems whose changes take certain directions.

Similarly, the <u>redefinabilities</u> of ideas in a single idea-system may be treated inductively as resemblances over time, and in terms of "logical moves," as above.

Perhaps the N-C approach may also give us an account of the nature of emergent qualities, especially the properties of organization not possessed by the components of a system (assuming these components to be defined historically, genetically, or in relation to some prior exposition.) The N-C account might treat these "organizational features" as the attainment in extension of some integrative or otherwise

significant property; where "significant" means "corresponding to some abstract model otherwise of interest to us." For instance, the development of a prestige hierarchy in a group that began as undifferentiated may be regarded as the extensional development of an emergent mathematical or abstract property whose remote predicates yield other logical characteristics. Thus we know from the mathematical model that if A has more prestige than B, and B more than C, then A will have more than C. This takes place while the system is maintaining similarities with the past, as above.

The N-C approach also would advocate a different style of approach to the more everyday varieties of "logic" in discourse and conversation. While the L&M approach urges people to "define their terms" and proceed in strict deductive development of their ideas, the CN approach would point out that this can be uneconomical, in terms of the effort required, and prone to formal errors which distract from the structure of assertion. It also throws discussion open to criticism for "classical fallacies," which formally disqualify the discourse. However, the CN* view regards this as misplaced attention. If logic is to be regarded as a useful activity, in which understanding is to be reached, it is much more important to search for the structure of assertion within a discourse, to find out what he means rather than what he says he means in an attempt at formalization.

* Don't change horses in the middle of the stream (discourse / symbols)

Naturally, considering the "gamesmanship" of logic, we may want to destroy the other fellow's argument, in which case forcing him into inappropriate formalization is just as good a ploy as staring intently at his shoes. The alternative may be regarded as too open-minded, looking too intently for "the good" in what he says. But concentrating on the structure of assertion by no means necessarily leads to a syncresis of everyone's views; if there is actual disagreement among discussants, it is best to understand this disagreement across the whole breadth of the opinions involved. Two people who understand each other's views fully can still disagree.

"Logic," not as a game but as reasonable behavior confronted by a problem, would also be treated differently by the CN approach. We think it is fairly clear that the need for formal logic in most non-mathematical reasoning problems is slight, except in puzzle books. By this view the examination of a problem leads much more directly out into Methodology, with less attention to formalization.

We have a number of suggestions for formal developments of the CN approach. For instance, a treatment of the possible logical moves when an idea or a model is disconfirmed might be of considerable interest. This we could call "logic in motion"--the study of the possible discrete changes in a logical system that will reduce mis-match with

the universe it is expected to describe. The study of what changes are minimal, adequate and dis-organizing would be one possible development along this line.

Similarly, a study of the logical moves necessary to maintain properties of the system, including remote predicates, would be interesting; this might be useful for describing the changes in an ideology which would cope with the "needs" (remote predicates) of an individual or an organization.

Similarly, the formal properties of logical moves that enable concepts to be sequestered from an otherwise integrated conceptual network might be a fruitful development. (The cook who has treated beer like wine or chicken stock, then learns that it turns scrambled eggs white and crumbly, must sequester this particular combination from his previously integrated concept.) This sequestering enables the system to remain highly integrated, in terms of the number of predications in the network, while still coping with the mis-match.

Similarly, the study of the impact of certain new predicational connections on a logical network might be a fruitful study. One practical application of this would be in the editing of motion picture film. The director is unable to synthesize all the possibilities in his mind, but he can make certain fragmentary decisions toward the

finished product, viz.: "If I use shot A, then I can't use shot B; if I let C run ten seconds, then I can't use the shot of her petting the dog..." Formal rules, if attainable, governing the further impact of leading decisions such as "I'd rather like to use A," would give a model capable of machine operation.

These are only a few developments of the approach. The present author, with his limited formal training and in an inappropriate setting, will be unable to pay attention to these developments for some time to come. However, their listing here may be of interest.

Let us now turn to "Systems Analysis." I believe that the problems of doing, or devising systems for handling and accomplishment, is in some sense isomorphic with the account given above of the network-configuration approach.

Today the world is confronted with greater and greater problems of planning and design, associated with large-scale engineering, electronic communication and data networks, and greater governmental capacity (wanted or unwanted) to influence all aspects of society.

"Systems analysis" is regarded by a number of people as jargon for "the problems of design and invention." We do not regard it as such. The problems of research in possible techniques, and the problems of integration of the parts of a solution, have reached a scale which totally changes the nature of doing things, in our view. In times past an

"invention" was the embodiment of a single principle, or the combination of a few, like the rubber eraser on the end of a pencil. Today things are different. A modern television-camera system involves the integration of optics, electronics, support and wheels, operator, and signal system to the operator. Modern computer systems involve optical reading and projection, printing methods, magnetic tape recording, card punches and telephone lines. Space ships are another obvious example. To consider the design of these a "single problem" is as mistaken as to consider them bundles of unrelated problems. We will not consider here the thesis that all problems of choice--moral, political, military, economic and tehhnological--are being progressively united into a unified field. Suffice it to say that the problems of systems, leading decisions, method and unity have reached a magnitude never known before.

In this article we will discuss a few topics within this field, and briefly sketch the analysis a CN approach might eventually perform.

Historically, one of the important branches of systems analysis has been the study of the different effects of different methods of doing things.* This has amounted to the study of the effectiveness of different "configurations" at doing the same task; for instance, the flight patterns of bombers, the scanning patterns of airplanes hunting submarines, and the positioning and inventory ppoblems of servicing

* The study of latent functions?

units. This study is called "Operations Research." Two features of its development have been the elaboration of statistical techniques for analyzing output, and the paradoxical use of curious ad hoc measurements of effectiveness.

One feature of immediate interest for the CN approach is the fact that in this study is the fact that these awkward metrics have often led to impressive savings and increases in efficiency (Hitch and McKean, in The Economics of Research in the Nuclear Age, mention the discovery by such methods that some minor safety device for a defending high-altitude fighter, was costing hundreds of thousands of dollars--an extravagance considering the losses that would attend the raid it was built to fight off, and the alternative uses of the money for defense allocation.) *

The partial equivalents, both of results and structure, input and output, demands and results, that different systems have are of obvious interest to the CN approach. Here some predicational connections (outputs, inputs, programs, field of operation) may show wide variations within "similar" networks.

This leads to another type of approach with some resemblances: the question, "What is a minimal list of the things necessary to do so-and-so?" leads to the study of the functional prerequisites for the functioning of some kind of system. A pioneering study was the famous article by Aberle, Cohen, et al., "The Functional Prerequisites of a Society,"

* Interesting problem: The consequences of being maximally efficient. The Pentagon, I think I recall, figures on 10-15% "waste" - what the consequences would be if such "slack" were eliminated?

and similar work has been done on the needs of social systems by Parsons and Bales. A point is in order here: the study of functional prerequisites is inductive, whether proceeding by the configurational-similarity method (as Parsons and Bales appear to) or a more informal enumerative method.

The last heading is that of most interest to us, the problems of explicit system-planning as they are today becoming clear. For instance, business methods in times past evolved by the success and elaboration of ad hoc procedures; which records needed keeping and which did not became worked out over time. For instance, rarely if ever is any record kept of the papers on a particular desk at a particular time. But modern data-handling methods require entirely new consideration of all phases of a process. Are multiple bills of lading necessary? Where, and how many, records shall be kept of shipments and inventory? How and when are they to be compared? What verification procedures are to be used to keep from being cheated? All of these things are open to question in a total systems design. (For a professional treatment of this topic, see Grad, "Business EDP on the Threshold," DATAMATION, January, 1962, pp.33-4.)

The old organizational features of systems for doing--let us call them "centralities"--are now open to total rethinking, [and a number of problems of the efficiency of parts]. Let us take examples of such centralities. There can be

centralities of power, like the mainspring of a watch, the power grid of an area, the engine of a car (which powers the generator.) There can be centralities of timing, like the pendulum of a clock, the phasing of alternating current, the central clock on a computer. There can be centralities of human coordination: thus an actor at a rehearsal need only look at the rehearsal area to see what part of the play is being worked on, and judge when he will be needed.

However, many of these centralities are now being reconsidered (for instance, the Philco 2000 computer is asynchronous, and does not run off a central clock.) The question is what alternatives or networks of alternatives will perform the same functions.

Then there are questions of the decomposition of task and function--will a given traditional breakdown of a problem of doing something last with new methods? Are some of the previous functions unnecessary?

There are questions dealing with emergent attributes of systems-for-doing which have developed informally and are not easy to re-think. For instance, the reward-and-bargaining systems current among workers in a department (see Blau, Dynamics of Bureaucracy, Part II) are quite independent of the original design of that department.

Then there are interesting questions of the evolutions

of systems for doing. All these systems develop within structural evolutionary constraints: for instance, IBM must make its new machinery compatible with the old, no matter how much they would prefer not to.

The versatility of systems under evolutionary constraint can be remarkable. For instance, the military system is adaptable, with hidden inefficiencies, to "all" tasks--but the army, the navy, and the Weather Bureau handle equivalent stations in quite different manners.

We have touched on these topics only briefly. Yet I hope it can be seen that the configurational-network approach may be fruitful for the description and induction of their correspondences and dissimilarities.

Nelson

p. 9. I think you run up against Gödel's Proof here — you can't start from nothing — postulate for everything. Some terms must be undefined (I am somewhat wried about Logic & 'common sense' —)

p.15 ff — This is what was done w/ the Aristotelian — Thomist analogy — & where did it get them? & The maintenance of broad, partial closure more important than giving them class T. a new, 'factors' system?

p.17 — This is the difference btw "hypothesis-comparison-test" & "GBO" analysis — the 1st T based on events in a system, the 2nd on a wholistic approach to the system as an entity.

This is a general guide for creativity, I think, rather than a deductive elaboration of the N-C (or CN) approach.